Coming to Terms with Establishing Operations

A. Charles Catania
University of Maryland Baltimore County

I was disappointed by Michael's article, and I suspect that my remarks will seem, at best, curmudgeonly. His earlier article on establishing operations was useful and well crafted (Michael, 1982); I drew upon it in the most recent edition of my text (Catania, 1992) when I added material on establishing operations and sharpened up my treatment of the establishing versus the discriminative functions of aversive stimuli in escape and avoidance procedures. I had hoped to find a more elaborated treatment in the present article. Instead I found only a little in it that had not already been presented more effectively in the earlier work, together with a number of problems.

I will comment here on some of the major issues that I found problematic, but I will also comment on a number of matters of detail. This may sometimes seem to be nitpicking, but that is because I believe such matters of detail bear on the standards we set for our students and on how those outside our field look upon what we do. I have no problem in general with the concept of establishing operations, but I do have a problem when new and potentially inconsistent usages are introduced without adequate justification and when our field is made to seem insular because we neglect the findings of those outside it. When our students go on to environments that are not exclusively behavior analytic, their survival may well depend on how well they recognize, in the integrative spirit of Keller

and Schoenfeld (1950), the legitimate contributions of others. With regard to these criteria, I do not believe that Michael's article sets a good example.

Before I move on to the more general issues, let me dispose of some of the matters of detail that troubled me. Michael's article introduces many terms and abbreviations without justification. Some of these substitute for existing terminology. I regard some of these novel usages as pedagogically or systematically unsound, and I believe that introducing so many new ones wholesale can only lead to terminological confusion. Furthermore, some also involve inappropriate characterizations of behavioral phenomena. I therefore begin with some remarks on the language of respondent conditioning and on a miscellany of other technical terms, and then move on to the language of establishment and its abbreviations. Some aspects of the latter discussion are relevant to and lead into the more general remarks with which this commentary closes.

Contingencies, Pairings, and Other Respondent Terms

The concept of contingency is of central importance in operant analyses, but it also plays a significant role in the analysis of stimulus–stimulus relations. Michael occasionally refers to respondent contingencies in terms of pairings. Yet it has been well established that the relevant variable in respondent conditioning is not number of pairings but rather contingency relations among stimuli (Rescorla, 1967; cf. Catania, 1992, pp. 192–194). For example, suppose that over some time period, each of 10 conditional stimuli is followed by an unconditional stimulus; this involves exactly 10 stimulus–stimulus pairings. Then suppose over the same time period, 100 conditional stimuli are presented, but only 10

are followed by an unconditional stimulus. Here again the number of stimulus–stimulus pairings is exactly 10. In the first case, however, the conditional probability of the unconditional stimulus given the conditional stimulus is 1.0, whereas in the second it is .1. These different stimulus–stimulus contingencies have very different behavioral effects, and the literature on the distinction between pairing (or association) and contingency is substantial. Given the attention that Michael gives to respondent phenomena, I was surprised that his article did not even hint at the distinction, the behavioral implications of which are highly relevant to the issues at hand.

Another, though admittedly more minor, pedagogical problem is with Michael's inconsistent usages of the terms *conditioned* and *conditional.* Aside from the unfortunate histories of these terms, *conditioned* is more and more often being restricted to respondent procedures (the expression *operant conditioning* perhaps is gradually becoming obsolete). To use *conditioned* in operant contexts seems a step backward, not least because it may make it just a little harder for students to learn to distinguish between operant and respondent procedures.

The literature on respondent phenomena includes many new and striking examples (e.g., lethal effects of drug overdose as a phenomenon of respondent conditioning; Siegel, Hinson, Krank, & McCully, 1982). Failures to distinguish contingencies from pairings could undermine the effectiveness of some applications (e.g., creating safety signals for children on intensive care units; Cataldo, Bessman, Parker, Pearson, & Rogers, 1979). The time has come to update our presentations to students and to dispense with old-fashioned examples such as pupillary conditioning (in fact, pupillary conditioning does not happen; Young, 1958).

A Terminological Potpourri

The point of this treatment of respondent terminology is that inconsistencies and ambiguities of terminology can sometimes lead us astray; they can especially raise problems for those approaching our field for the first time, whether they be our own students or colleagues from other approaches. Because Michael's article includes so many terminological innovations, it seems particularly important to examine his terms and the relations among them in considerable detail. Here are a few examples.

Michael has coined the word *motivative.* I have been unable to find it in dictionaries, and *motivational* already exists as a standard usage. I see no case in which Michael uses *motivative* where the existing standard would not be perfectly appropriate. If it might put off a reader from outside behavior analysis, why do we need it?

Michael occasionally refers to pain as a stimulus, but that is not a proper usage. Experimenters who study aversive control do not present pains to organisms.

Michael introduces *abolishing operations* as a new term, but does not acknowledge the different earlier usage (in the sense of abolishing a discrimination) in Keller and Schoenfeld (1950).

Michael uses the term *reinforcement* as if its definition included that of *reinforcer*; a parallel distinction between process and stimulus also exists for *punishment* and *punisher.* These distinctions have been standard in our field for roughly a quarter of a century, and the further usage of reinforcing responses rather than organisms has the practical advantage of discouraging ambiguous statements about the relations between reinforcement procedures and the responses to which they are applied. I was surprised to see that Michael did not take into account the important distinctions that are implicit in these standard usages.

Michael introduces the abbreviations UE, CE, and UC as unconditioned elicitor, conditioned elicitor, and unconditioned conditioner, respectively. These terms are used in just a few places, so why, especially in the context of other abbreviations with which they will easily generalize (i.e., EO, UEO, CEO), offer

substitutes for an existing terminology with wide currency (US, CS, etc.)? Any disadvantages of that earlier terminology are counterbalanced by their accessibility to those who have learned the vocabulary of conditioning in contexts other than behavior analysis. Again, the issue of the accessibility of behavior-analytic materials to those outside of behavior analysis is relevant.

Michael proposes to use upper and lower case superscripts to distinguish conditioned and unconditioned reinforcement and punishment. This seems too major an innovation to introduce in passing; a full treatment of this one terminological point would be more appropriate for the "On Terms" section of this journal (some comments below on abbreviation may also be relevant).

Technical terms typically have multiple functions. I have already implied that terms can be coined to make their mastery easier for the uninitiated undergraduate, or to make our literature more accessible to colleagues outside our field, or to accommodate important distinctions among phenomena. But part of my problem with the vocabularies that I find in this article is that they seem to be inconsistent with respect to pedagogical and professional objectives.

In some places, Michael adopts terms from everyday vocabulary, as when he speaks in terms of wants, or as when he introduces as purportedly technical terms the unparallel *improvement* and *worsening* (it is not clear why *bettering* and *worsening* would not have done just as well). He then sometimes goes on to define these terms behaviorally. But elsewhere, without definition or explanation, he introduces such technical expressions as *activation syndrome* and *intrinsic motivation*. In other words, this article seems to be torn between colloquial and technical usages, presumably for different purposes, and as a result creates difficulties in both arenas. The various problems of terminology itemized above provide illustrations; with this background, it may now be appropriate to move on to the vocabulary of establishing operations.

The Language of Establishment and Its Abbreviations

We must take care whenever we transform colloquial terms into technical ones. For example, if a student talks about shaping as a procedure for establishing an operant class, can we appropriately rule out this usage of *establishing*, even though it is perfectly acceptable in other colloquial contexts? Certainly we will continue to speak about establishing a point in an argument or establishing the facts of some matter. In some cases, we already have other vocabularies as alternatives to that of establishment (e.g., shaping or differentiating an operant class); in others, the general term might be useful (e.g., establishing a stimulus as discriminative). But the language of establishing operations is already strongly enough established (sic) that it would probably be inappropriate to modify its present primary usage.

Michael makes substantial use of abbreviations, so it may be worthwhile to consider their functions. Certainly they are sometimes convenient, especially if long terms are heavily used. But they also place a burden on the reader, and so may not be justifiable for terms used only occasionally. For example, abbreviations rarely save much in the way of journal space; the difference between terms abbreviated and terms written out in full will sometimes make an article run over an additional page, but rarely more than that. Thus, abbreviations should serve some function other than space saving. Perhaps most important, they must also have mnemonic properties that help the reader.

I found Michael's abbreviations to be particularly cumbersome. Consider the difficulty of reading passages involving EO and UEO and CEO and UE. Not only is the reader who must translate each abbreviation as it is encountered slowed down, but the mostly vowel makeup of these abbreviations makes it all too easy to mix them up. They provide great material for objective examination questions, but that is not what effective ter-

minology (or effective teaching) is for. We need teach only those abbreviations that a student can be expected to encounter in relevant literature (e.g., the student of reinforcement schedules must be able to interpret VI, VR, FI, FR, and so on). Why then create new ones? Why subject students or colleagues to such an unfriendly set of abbreviations?

But UEOs and CEOs also involve other problems; these may even have been obscured by the abbreviations. Establishing operations are either experimental manipulations or parallel environmental properties (e.g., water deprivation, which may be experimentally arranged or may occur in a natural environment). Yet the terms *unconditioned* and *conditioned* are not appropriate modifiers for establishing operations; these operations are not unlearned or learned. Michael is instead concerned with the reinforcing properties of relevant stimuli (in this case, water). These properties may be unlearned or learned, but surely the operations are not subject to this distinction. The terms *unconditioned* and *conditioned* should modify the reinforcing properties of the relevant stimuli and not the operations that produce those reinforcing properties.

Another potential source of confusion is implicit in the distinction between UEOs and CEOs. The distinction appears to correspond to that between learned and unlearned, but the term *conditioned* is much narrower than that. Consider imprinting as an establishing operation. The establishing event is the presentation of the imprinted stimulus early in the life of a duckling. Presentation of this stimulus then reinforces arbitrary responses, such as pecking on a key or standing still on a platform; in natural environments, it usually reinforces following (cf. Peterson, 1960). Some stimuli become more easily imprinted than others (e.g., moving stimuli are more effective than stationary ones), but the imprinted stimulus still depends on what the duckling is exposed to. In other words, the imprinting depends on exposure, or is learned, or, at least is not,

in Michael's terms, unconditioned (in the sense that it is conditional on what has been presented to the duckling). But the language of *unconditioned* and *conditioned* carries the suggestion of respondent procedures, whereas imprinting is not an example of respondent conditioning. In this case, I would argue that the terms *unlearned* and *learned* are actually more effective and less likely to be misunderstood.

Establishing Operations Within Behavior Analysis and the Field of Motivation Outside It

Michael suggests that establishing operations have been ignored within behavior analysis, but a substantial literature on the relativity of reinforcement has as its primary concern the study of manipulations that change reinforcing properties (e.g., Premack, 1971). Even more important, such accounts deal with these establishing operations (even if called by other names) not in terms of stimuli but rather in terms of behavior. The relative probabilities of the responses that are enabled by different consequences determine relative reinforcing effectiveness; raise the probability of the behavior enabled by a stimulus (e.g., the probability of drinking given water) and you raise the effectiveness of that stimulus as a reinforcer. This account comfortably accommodates many of Michael's examples, such as the reinforcing effectiveness of an opportunity to engage in "elicited aggression" given aversive stimulation.

Reinforcement relations can be reversed by changing the relative probabilities of responses (as when appropriate deprivations of opportunities for drinking or for running can make either the opportunity to drink reinforce running or the opportunity to run reinforce drinking). This account includes both the reinforced response and the responding occasioned by the reinforcer, and in so doing easily handles decreases in the effectiveness of reinforcers, whereas the language

of establishing operations does so less naturally (disestablishing a reinforcer?).

Such relativistic accounts can also be extended to aversive events. For example, the probability of responses produced by aversive stimuli is raised by presenting those stimuli, and this operation raises the reinforcing effectiveness of the termination of those stimuli. There is the potential here for an account of aversive control based on behavior; it would be of interest to develop the implications for the aversive side of Premack's relativity account. There is also a literature relevant to the relativity of traditional respondent USs and CSs, which Michael seems to allude to but fails to develop.

Those outside behavior analysis sometimes see our field as insular, and that view is strengthened whenever such insularity is displayed in our writings or by our students. It is therefore important to relate behavior-analytic work to work done in other fields (the example set by Keller & Schoenfeld, 1950, has already been cited). For this reason, I was particularly concerned by Michael's treatment of the field of motivation. Even a cursory review of that field in standard texts on motivation or physiological psychology (e.g., Beck, 1990; Carlson, 1991) demonstrates that Michael's account of UEOs is far too superficial. For example, with regard to sexual reinforcers, he states that "there do seem to be deprivation effects for both sexes" (p. 195). He need not speculate: The literature on sexual behavior is substantial. And specialists on hunger would note that, in contrary to Michael's suggestion but consistent with Skinner's own treatment, hunger pangs do not constitute a good example of stimuli that "evoke" eating, whether they are treated as discriminative or as establishing. A lot is known about the variables that are effective as establishing operations in the areas that are traditionally spoken of as hunger, thirst, sexual and maternal behavior, thermoregulation, conflict, aggression, sleep, and addiction (this list is not exhaustive). The specialist from any of these areas who

had to judge what behavior analysts know about them from Michael's article would probably come away with a bad impression.

In summary, I believe that we could increase the salience of establishing operations in behavior analysis by studying the relevant literature outside of behavior analysis, and then organizing it in the context of a classification of establishing operations. This would be an important contribution. The claim that by the late 1960s the behavior-analytic literature no longer gave much attention to motivation, however, is problematic in two ways: First, it fails to note relevant work, such as Premack's, that was relevant to motivation but that was not so labeled; second, it does not address the large body of work outside of behavior analysis in the area of motivation. Michael's article, which makes only occasional reference to the literature outside of behavior analysis, could have performed a truly significant service by surveying and interpreting that literature in behavior-analytic terms. Such an article might have achieved an objective of perhaps equal importance to that of raising the interest in these issues on the part of behavior analysts; it might have illustrated to those outside of behavior analysis the value of the behavior-analytic enterprise and the range of phenomena to which it is applicable. As it is, it only shows how parochial our approach can sometimes be. I wish that Michael had given us a synthetic treatment of behavior analysis and the existing field of motivation. He is well qualified to do so, but I do not think that is what he has given us here.

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